exhibited in the quaternionic notation as in the other. Moreover, we can write $\sigma.\Phi$ for $\sigma.(\alpha\lambda + \beta\mu + \gamma\nu)$. This represents a vector which is a function of σ, viz. the function conjugate to $\Phi.\sigma$; and $\sigma.\Phi.\rho$ may be regarded as the product of this vector and ρ. This is not so clearly indicated in the quaternionic notation, where it would be straining things a little to call $S\sigma\phi$ a vector.

The combinations $\alpha\lambda$, $\beta\mu$, &c., used above, are distributive with regard to each of the two vectors, and may be regarded as a kind of product. If we wish to express everything in terms of i, j, and k, Φ will appear as a sum of ii, ij, ik, ji, jj, jk, ki, kj, kk, each with a numerical coefficient. These nine coefficients may be arranged in a square, and constitute a matrix; and the study of the properties of expressions like Φ is identical with the study of ternary matrices. This expression of the matrix as a sum of products (which may be extended to matrices of any order) affords a point of departure from which the properties of matrices may be deduced with the utmost facility. The ordinary matricular product is expressed by a dot, as $\Phi\,.\,\Psi$. Other important kinds of multiplication may be defined by the equations—

$$(\alpha\lambda) \overset{\times}{\times} (\beta\mu) = (\alpha \times \beta)(\lambda \times \mu), \qquad (\alpha\lambda) : (\beta\mu) = (\alpha.\beta)(\lambda.\mu).$$

With these definitions $\frac{1}{6}\Phi \overset{\times}{\times} \Phi : \Phi$ will be the determinant of Φ, and $\Phi \overset{\times}{\times} \Phi$ will be the conjugate of the reciprocal of Φ multiplied by twice the determinant. If Φ represents the manner in which vectors are affected by a strain, $\frac{1}{2}\Phi \times \Phi$ will represent the manner in which surfaces are affected, and $\frac{1}{6}\Phi \overset{\times}{\times} \Phi : \Phi$ the manner in which volumes are affected. Considerations of this kind do not attach themselves so naturally to the notation $\phi = \alpha S\lambda + \beta S\mu + \gamma S\nu$, nor does the subject admit so free a development with this notation, principally because the symbol S refers to a special use of the matrix, and is very much in the way when we want to apply the matrix to other uses, or to subject it to various operations.

J. WILLARD GIBBS.

New Haven, Connecticut.

The Meaning of Algebraic Symbols in Applied Mathematics.

PROF. GREENHILL, on p. 462 (March 19), gives a naïve and most instructive description of the straits to which a "practical man" is put when he wishes to interpret the simplest general formula.

I have always held, not in sarcasm but in sorrow, that students brought up on the system of specialized and limited numerical formulæ used by Prof. Greenhill and some other Professors of Engineering in this country, must necessarily go through the tentative trial-and-error sort of process which he so graphically describes, whenever they have to obtain a numerical result from anything not already arithmetical. In other words they are unable to deal with complete algebraic symbols or concrete quantities.

The symbol "v" to them does not completely represent a velocity, it only represents a number; and to make it represent a velocity some words, such as "in feet per second," must be added. Whereas, since it is plain that the velocity of light does not vary with its numerical specification, nor the size of a room change according as it is measured in feet or in inches, it is surely better to make a symbol express the essential and unchanging aspect of the thing to be dealt with, *i.e.* the thing itself, and not merely the number of some arbitrary and conventional units which the thing contains.

May I, then, assure Prof. Greenhill very seriously, *and with entire appreciation of and accord with his insistence that all expressions should be complete and capable of immediate practical numerical interpretation*, that the equation $T = \rho v^2$ is perfectly complete, and that it is true and immediately interpretable in every consistent system of units that has been or that can be invented? T is the tenacity, ρ the density, of the material of a ring, and v is its critical or bursting velocity. There is no need to say a word more. And no properly taught student ought to have the slightest difficulty in obtaining a numerical result directly in any system of conventional units that may be offered him.

It is the frequent recurrence of such ghastly parodies of formulæ as—

$$T = \frac{62{\cdot}4}{2240 \times 144} \cdot \frac{\rho v^2}{g}$$

in many engineering treatises which makes them such dismal reading. It is a standing wonder to physicists how a man of Prof. Greenhill's power can fail to see the inadequacy and tediousness of expressions which are only true in one particular system of units, and which to be true even in that require the special statement of every unit employed. For not only is Prof. Greenhill's expression long in itself, but it is incomplete without the tiresome addition, "ρ being measured in so and so, v in &c., T in something else, and g meaning nothing more than the pure number 32·2." All this has been needlessly put into the formula, and so has to be wearisomely taken out again.

If there be any physicist who does *not* contend for the concrete interpretation of algebraic terms (wherein each symbol is taken to represent the quantity *itself*, and not merely a numerical specification of it in some conventional unit—see, for fuller explanation, NATURE, vol. xxxviii. p. 281), I trust he will write and uphold his position on the side of Prof. Greenhill.

I suspect that the cause of Prof. Greenhill's failure at present to recognize the extreme simplicity and reasonableness of the physicist's procedure is to be found, partly in a vague idea that in order to get numerical results in British units from a general expression it is necessary to work it out in C.G.S. units first, and then translate, which I assure him is not the case; and partly in the general difficulty which most people feel in thinking it possible that they can be mistaken.

I would gladly convince Prof. Greenhill if I could, because he would carry with him so many other teachers, and thus a mass of waste labour would be saved annually to several thousands of students. Would it be too much to ask him to consider the matter with care, and, if possible, from our point of view; setting me a sum to do if that would be any assistance towards bringing him to the desired point of view?

OLIVER J. LODGE.

Tension of a "Girdle of the Earth."

IT is perfectly true, as Prof. Lodge has asserted, that a cord or chain running on its own track as an endless band in a frictionless groove *of any form* will not require the sides of the groove to keep it in that form. But whatever velocity it moves with, such a tension will exist all round it as to resist the centrifugal forces of its windings; and to preserve them by virtue of the curvature and constant tension, invariable in shape however the speed of coursing of the belt may be increased, without any external guidance and assistance. If w is the cord's mass per unit of its length, and v the velocity with which it pursues its course, wv^2 is the tension in dynes which will be set up all round it. The speed may of course be so increased as to tear the cord or chain to pieces; and this will occur in steel tires of railway wheels, for example, if the train's velocity on which the wheels are carried is much more than 120 miles an hour. Mr. Bourne long ago pointed out, in his works on the steam-engine, that a very low limit of speed in railway trains is enforced for safety in view of this dynamical condition so as not to approach and exceed working and proof-stresses at least in the material of which steel wheel-tires are formed.

But the truth of the proposition $T = wv^2$ rests entirely on the supposition that the running cord or cable pursues *exactly its own curve* in its motion. For a tension of 30 tons per square inch to be reached in a maritime cable in virtue of its being carried round either at the equator or at any distance of latitude from the equator, it must be presupposed that while buoyed with its own submerged lightness so as to be practically weightless in the water, it must from one end point of support to the other *follow accurately* the equator's circle of curvature, or the circle of curvature of the small circle of latitude along which it is laid, because this is *the line of motion* along which its parts are carried along by the earth's rotation. These circles are practically straight lines for any mile or two of cable, and truly enough, if in the presence of even the weak force acting "centrifugally" on the cable's mass by the earth's rotation, it is attempted (supposing it to be quite weightless otherwise) to pull it as nearly straight as the hardly sensible curvature of the earth requires, mathematics would not yield its point an inch, and a pull of

© 1891 Nature Publishing Group

something like 30 *tons per square inch would be required.* If the natural weight of the cable is nearly 400 times as great as the counter-force on it produced "centrifugally," we could imitate this outwards, by supposing the earth to revolve about 20 times faster than it is actually doing; and then the end pulls required to make a telegraph cable relieved from gravitation and acted on only by this outward "centrifugal force" just equal to the cable's weight, to stretch it to as flat a curve as that of the earth's circumference and of its circles of latitude, would be about 20 × 30 tons per square inch, or vastly more, of course, than any cable could withstand! But what telegraph-engineer would ever dream of trying to stretch telegraph wires on poles along the banks of a canal so that even between poles they should everywhere *be absolutely parallel to* the water level surface in the canal? With their natural weight acting outwards against him he would know that the attempt would be sure to result in rupture! And even if the centrifugal force due to the earth's velocity in this latitude of about 1250 feet a second acted alone on the wires instead of the above supposed one of about 5 miles a second to represent an equivalent to gravity, he would surely not be surprised to find his attempt to pull the wires as straight as the surface of still water, end only unsuccessfully in breaking them?

A. S. HERSCHEL.

New Athenæum Club, London, March 20.

Spinning Disks.

MR. WEHAGE, of Berlin, has independently obtained Prof. Ewing's results for a solid disk by putting $p_1 = p_2$ for $r = 0$, in Grossmann's equations.

As Prof. Boys points out, Maxwell attacked the problem in 1850, at the age of 19. I have gone over his solution ("Papers," vol. i. p. 61), and find that, in consequence of two slips he has made, his results do not agree with Ewing's. His first equation of (57) should read $\frac{c_1}{2r^2}$, instead of $\frac{c_1}{r^2}$; and his third equation of (57) will then read $-\frac{c_1}{2r^2}$, vice $-\frac{c_1}{r^2}$. His final result (59) is not affected by this alteration, which only changes c_1 and not c_2; but, owing to a wrong sign, that equation should read

$$q = h_1 + \frac{\pi^2 k}{2gt^2}\left[-2(r^2 + a_1^2) + \frac{E}{m}(3r^2 - a_1^2)\right];$$

which, remembering that Maxwell's E and m are E and $E/(1+\mu)$ respectively in Ewing's notation [or Thomson and Tait's $9nk/(3k + n)$ and $2n$, respectively], is seen to be the same as

$$p_1 = h_1 + \frac{\omega^2\rho}{8}\left[(3 + \mu)a^2 - (1 + 3\mu)r^2\right];$$

except that Maxwell takes tensions negative.

Maxwell's equation (60) is correct; but is not deducible from his (59).

As Prof. Pearson remarks (Todhunter and Pearson's "History of Elasticity," vol. i. p. 827), Maxwell seems to have thought the disk would yield first at the rim.

Cambridge, March 23. J. T. NICOLSON.

P.S., March 28.—Since writing the above, I have read Prof. Pearson's letter (NATURE, March 26, p. 488), in which he says that "Grossmann's results, such as they are, flow at once from Hopkinson's corrected equations." They flow, however, also at once from Maxwell's equations, if these are corrected in the manner I have shown above. Prof. Pearson is not correct in supposing that Maxwell's and Grossmann's solutions lead to different results for the position on the disk of the maximum hoop-tension. Maxwell probably thought the disk would yield first at the rim from his putting $r = a_1$ in his final result; but his corrected results are identical with Grossmann's for a hollow, and with Ewing's for a solid disk.—J. T. N.

The Stresses in a Whirling Disk.

BY an unfortunate accident my letter on this subject was printed (NATURE, March 19, p. 462) without correction in proof. There are several *errata*, due to slips in the manuscript.

In column 1, p. 462, line 10 from bottom, the words "hoop" and "radial" are transposed. Read "the hoop tension p_1 and the radial tension p_2."

In column 2, equation (7) should read

$$\frac{u}{r} = \frac{C}{r^2} + C_1 - \frac{(1 - \mu^2)\,\omega^2\rho r^2}{8E} \quad \ldots\ldots (7)$$

Further down, the expression for the maximum radial tension in a disk with a central hole should read

$$\text{Max. } p_2 = \frac{\omega^2\rho}{8}(3 + \mu)(a_1 - a_2)^2.$$

And in the special case when the hole is very small,

$$\text{Max. } p_1 = 2 \times \text{Max. } p_2 = \frac{\omega^2\rho a_1^2(3 + \mu)}{4}.$$

Cambridge, March 20. J. A. EWING.

The Poison Apparatus of the Heloderma.

OUR largest United States lizard, the *Heloderma suspectum*, is too well known to science to require any special description here. In the Proceedings of the Zoological Society of London, for April 1, 1890, the writer published quite an exhaustive memoir upon the entire morphology of this famous reptile, and among other parts of its anatomy reference was made to its poison apparatus. Fig. 4 of Plate xvi., of the contribution in question, showed a superficial dissection of the under side of the head of a large Heloderm, and upon the left side of the same the submaxillary gland is turned outwards, thus rendering it possible to be seen the four structures leading from (or to) this gland to separate and as many foramina opposite them, which exist upon the external aspect of the ramus of the lower jaw. Heretofore, these four structures have very generally been looked upon as the four poison ducts leading from this gland to the hollow space in the mandible, which latter in turn had its upper outlets in the minute foramina, found one each at the base of the teeth of this jaw. The poison was supposed to pass from the gland through these four ducts into the body of the ramus, then through the above-noted foramina, whence it was conducted into any wound the reptile might inflict with its teeth, along the grooves which have for many years been known to exist upon them. Without making any especial microscopical description, this is practically the view I supported in my memoir in the Proceedings of the Zoological Society, and I added that "Fischer found in his specimen that these ducts branched as they quit the gland; this was not the case in the reptile examined by me. Each duct passes obliquely upwards and inwards through the lower jaw, and its internal opening within the mouth is found at the base of the tooth it supplies, near the termination of the groove of the tooth" (p 207). Dr. Fischer's paper was published in Hamburg in 1882, and in it he also gives a figure showing the ducts I have just mentioned; and he is largely responsible for the view that has been adopted in regard to them As early as 1857, however, John Edward Gray evidently leaned in the same direction, and he speaks of "*Heloderma horrida*, in which all the teeth are uniformly furnished with a basal cavity and foramen,"—structures which he compared with the poison fangs and associated parts of venomous serpents. Dr. C. K. Hoffmann had the same ideas about the poison apparatus of the *Helodermas* (Bronn's "Klassen des Thier-Reichs," Bd. vi., iii. Abth., 30-32 Lief., pp. 890-892), and he republished Fischer's figures; and thus running through the similar views of numerous other leading herpetologists, we find, even as late as 1890, Prof Samuel Garman, of Harvard University, quoting Fischer's description of the poison glands of *Heloderma* without question (author's reprint from Bull. Essex Inst., vol. xxii., Nos. 4-6, 1890, p. 9), and he adds that "no glands have been found on the upper" jaw. With all this in my mind, I was not a little surprised when Prof. C. Stewart, on January 20 last, in a paper read before the Zoological Society of London, claimed that he believed "that he had shown that in both species [*H. horridum* and *suspectum*] the ducts of the gland did not enter the lower jaw, but passed directly to openings situated under a fold of mucous membrane between the lip and the jaw. He thought that the structures previously described as ducts were only the branches of the inferior dental nerve- and blood vessels." Upon hearing this, I at once took a large specimen of *Heloderma suspectum* to my friend Dr. E. M. Schaeffer, the well known microscopist in Washington, D.C., who kindly examined the structures, and found them to be exactly as Prof. Stewart had described. It now remains to be said of what use are the foramina at the base of the teeth in these lizards. Why are the upper teeth grooved when there is no poison gland upon the upper jaw? Would not such a severe bite as a *Heloderma* is enabled to give kill "frogs and insects," even were no poison injected into such wounds? Are the grooves on the teeth there to conduct a poison into the wounds

© 1891 Nature Publishing Group